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Revisiting Women And Feminism In Developmental Biology

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Figure 4.1 shows eight biologists—three women and five men—sitting together at a lecture at the annual meeting of the Society for Developmental Biology in 1998. This is an interesting photograph if only because it shows a fairly equal representation of senior men and women at a scientific conference. But this picture could also represent a poor sample, a subset too small to represent the entire group. Indeed, it is: it is a picture only of the recent presidents of the society.

Women appear to have done extremely well in developmental biology, both in scientific research and in ascending its professional ranks. As Evelyn Fox Keller has noted, "it is the intellectual space occupied by women in developmental biology today that has led to the subjective impression among some biologists that developmental biology is a field now dominated by women."[1] Most prominently, the first Nobel Prize awarded to developmental biologists in fifty years went in 1995 to Christiane Nüsslein-Volhard (who won the prize along with her colleague Eric Wieschaus and the geneticist Edward B. Lewis), and the first March of Dimes Award in Developmental Biology went jointly to Beatrice Mintz and Ralph Brinster in 1996. Of the fourteen members of the present executive board of the Society of Developmental Biology, nine are women, including its president and seven of the nine members-at-large. Any discussion of who are the most influential developmental biologists in the world would include (but certainly not be limited to) such names as Kathryn Anderson, Cori Bargmann, Ruth Bellairs, Marianne Bronner-Fraser, Connie Cepko, Marie Di Berardino, Elizabeth Hay, Brigid Hogan, Vivian Irish, Laurinda Jaffe, Cynthia Kenyon, Judith Kimble, Nicole Le Douarin, Ruth Lehmann, Gail Martin, Anne McLaren, Barbara Meyer, Lee Niswander, Virginia

Figure 4.1 Former presidents of the Society for Developmental Biology assembled during the Conklin Lecture at the 1998 annual society meeting: (*top, left to right*) Dave McClay, Matt Scott, Chuck Kimmel, Helen Blau, Janet Rossant, (*bottom*) Kathryn Anderson (appearing only partially), Alan Spradling, Meredith Runner. Photograph taken by Laurie Iten for the society's website.

Papaiannou, Liz Robertson, Janet Rossant, Carla Schatz, Trudy Schüpbach, Irma Thesleff, Cheryll Tickle, Shirley Tilghman, Kathryn Tosney, and Virginia Walbot.

Like any important and anomalous observation in science, the apparent success of women in developmental biology suggests more questions than it answers. For though the number of women who have recently received assistant professorships in this field is remarkable, the total number of women practitioners is still under 50 percent, as Keller also notes. Thus the most basic questions are: What constituted the success of women developmental biologists and how did it come about? In the era from 1930 to the present, when feminists have been increasingly concerned about professional gains made by women in science, how did developmental biology attract and support a relatively large number and variety of women? How did particular individuals negotiate careers as developmental biologists in ways that allowed them to be perceived as leaders in this field from its start, and did these strategies and perceptions change over time? Another, more complicated question follows from this line of inquiry: namely, how have the number and achievements of women in developmental biology during this period made a difference? Have these women made developmental

biology a "feminist science"—or has feminism changed the means by which we do developmental biology in other ways?

In her essay "Developmental Biology as a Feminist Cause?" Keller addresses many of these issues. Keller suggests that the large number of women in developmental biology "has a lot to do with timing."[2] For the period since World War II, she cites the coincidence of increasing numbers of women in science and the rise of developmental biology as a field (though the disciplinary label itself only dates from the 1960s).[3] For the earlier period, she notes that the type of scientific work developmental biology required "was hard, often back-breaking work and widely assumed to be unrewarding. What more natural job to assign to women?"[4] The first goal of our chapter is to further contextualize the history of women developmental biologists in relation to specific practical and institutional circumstances in biology—both before and after World War II—and suggest some additional areas for exploration.

We also want to revisit the issue of the meaning of gender in the history of developmental biology in order to inquire how it might be investigated further. Along these lines, Keller argues that the career of Nüsslein-Volhard illustrates the potency of the cultural symbolic work of gender in the history of developmental biology. Nüsslein-Volhard, she demonstrates, possessed a "multifaceted ambivalence" about feminism and the transformation of scientific career tracks in order to accommodate or encourage women. But it was precisely her ambivalence that situated her to make an "intervention of immense value to women in science"—specifically, as a mentor to some American women developmental biologists and as a researcher who sought to restore investigative prominence to the role played by the egg's cytoplasm in gene activation. "Nüsslein-Volhard," Keller writes, "stood at the intersection of multiple crossroads, able to make remarkably productive use of the ambiguities of her location in large part because of the timing of her intervention."[5] Using our own brief case studies of Salome Waelsch and C. H. Waddington, we argue that Keller's emphasis on the power of multiple "situatedness" for women developmental biologists might be broadly generalizable to early practitioners in the field as a whole. Thus we suggest that gender would be a potent historical tool for exploring the social and intellectual history of developmental biology as it relates to the broader history of twentieth-century biology, as well as to the lives and work of individual scientists.

Ultimately, we discuss the historical intersection of late-twentieth-century feminism with developmental biology and point to how the knowledge critiques that resulted transformed the field. But just as

there is no one feminism, there is no one feminist critique of science, nor is there any one reason for any particular woman to enter science or any field of science. What attracts one woman to a science may repel another. A feminist scientific agenda of one age might be the reactionary agenda of a different age. We conclude, then, that while these critiques are the best places to look for the difference that feminism has made thus far in developmental biology, much historical and sociological work remains to be done on the fate of feminist ideals in both the theory and the practice of this growing scientific discipline.

WOMEN, EMBRYOLOGY, AND GENDER BEFORE WORLD WAR II: A DYNAMIC OF INSTITUTIONAL AND SOCIAL RESOURCES

We should look first at the issue of how women first came to occupy the field of developmental biology.[6] What historical conditions might have allowed women to find this particular niche in the sciences? That is, how would women be informed that there even was such a field as developmental biology?

The answer to this question may change dramatically with the politics of the times, but in early-twentieth-century America, there were no obvious intellectual incentives in the standard public school curriculum for girls or women to learn about research in embryology/developmental biology. Developmental biology is not a subject that has ever been well integrated in high school biology books. Indeed, probably very few of us were taught developmental biology in our high schools, because to teach developmental biology means teaching sex, and we cannot do that in America. Contemporary developmental biology is a niche more likely to be presented in media than in textbooks and talked about more in schoolyards than in classrooms. High school biology books are characterized by gorgeous pictures and superficial discussion. The Biological Science Curriculum Study (BSCS), which has published some of the most important high school biology textbooks in the past thirty years, set up its first developmental biology advising group as late as 1999. The title of the vanguard BSCS book, though, is no longer called *From Molecules to Man.*

One explanation that needs to be empirically investigated is whether the social and material situatedness of women's bodies in any way contributed to women's entering this field. Development from the human zygote to the newborn human being is a process that takes place within the body of a woman and that never happens within the body of a man. To the extent that having a vagina, ovaries, and a womb

has been ideologically important in Anglo-American culture, and to the extent that being fertile has been considered important to the family and the nation, embryology could have been construed most literally as "women's work." And if one were already training in biology and looking for a field in which to specialize, one might ask, "Are the questions of this field fundamental and important questions?" Because of her specific cultural location, a middle-class woman coming of age in the early twentieth century might perceive the questions of embryology to be important and worthy of further investigation—much in the same way that many women were drawn to eugenics research "by sympathy with its ideals."[7]

But once there, what conditions might have allowed women to find this field more comfortable than other possible scientific fields? Here other obvious reasons emerge which concern the unique institutional configuration of embryology and its corresponding place in the professional hierarchy. At the turn of the last century, teaching was considered a role where women could influence the world; it certainly gave women public responsibility and got them out of the home. As Margaret Rossiter has pointed out, natural history and its teaching became open to women in the 1870s, and embryology was seen as being an excellent and accessible entry into the world of nature.[8] The opening of a chick's egg each day during its three-week incubation provides a wonderful view of development, as does the metamorphosis of tadpoles and caterpillars. Embryology has claimed a large number of women practitioners since its inception in America, and this seems to be intimately connected with biology education. The Marine Biological Laboratories (MBL) at Woods Hole was founded by collaboration between the Boston Society of Natural History and the Women's Education Association of Boston.[9] The embryology courses at the MBL were evenly filled by men and women (although the instructors were routinely male).

But though embryology was initially considered to be one of the most important elements of natural history,[10] women began to be excluded from this and other sciences when the urge to professionalize swept academia in the 1890s. The MBL was no exception to this trend, even though the women scientists there were already well established. The women from Goucher, Mount Holyoke, and Bryn Mawr would still come to the MBL, but they did not get positions in the prestigious universities; instead, they brought natural history into high schools and women's colleges. Not insignificantly, they also brought their expertise into their husband's laboratories. E. B. Wilson, T. H. Morgan, E. Conklin, F. R. Lillie, and E. N. Harvey each found his wife-to-be

at the MBL. Conklin wrote that marriages might be made in heaven, "but there is certainly a large branch office in Woods Hole."[11]

Relatedly, in the 1920s, classical genetics displaced embryology from its position of being the major biological science explaining heredity, and this newer, more reductionist discipline was almost entirely male at its cutting edge. Looking at T. H. Morgan's laboratory, which was to become the paradigm for genetic research centers, Robert Kohler notes, "Wives of graduate students worked as technicians and stockkeepers. So the village society of the drosophilists was not monkishly male, but women did not occupy official positions; they were there as unpaid working wives and volunteers. They do not appear in official photographs. The group's formative psychosocial relationships were male: master and disciple, father and son, Boss and the 'boys.' "[12] Morgan did have some women graduate students, but they were placed on peripheral projects (not the gene-mapping one) and published fewer papers than the "boys." Thus in the first three issues of *Genetics* (starting in 1916), there are no women authors. The sole woman author in volume 4 is Clara Lynch, a doctoral student of Morgan's who was doing her thesis on interspecific sterility and who later left drosophila genetics to pursue work at Rockefeller University on what Kohler has called the "messier aspects" of genetic problems in mice. But even those who began wanting to work on "messy organisms" did not fare much better. As late as 1928, the president of Harvard rejected the application of a Miss Warmbier to the Bussey Institution—Harvard's preeminent mammalian and plant genetics research center—on the grounds that her place might be more productively filled by a male student.[13] In short, genetics research was at the forefront of American life sciences both intellectually and professionally, and with the prominent exception of eugenics fieldwork, women were difficult to find.[14]

In turn, embryology was marginalized and lost its former prestige.[15] Until 1995, only one embryologist (Hans Spemann) had received a Nobel Prize. In many ways, it may be comparable to X-ray crystallography, another field that was considered peripheral, full of material details, and full of women practitioners. With genetics attracting the men (who, after all, were considered the employable members of society), embryology was left to women, who could get positions at teaching colleges, women's colleges, and private foundations or research institutions.

It would also be interesting to determine if the material culture of embryology further contributed to women's professional advancement in ways that other life science practice could not. For example, dependence on animal breeding seasons presents potential pushes and pulls

for women entering this field. Until the current age of molecular techniques—that is, before today's professors got their positions—embryology was not an easy subject in which to make a reputation. If you wanted to study amphibian development, you waited until spring, went out into the woods, collected the freshly laid eggs, and did your experiments as fast as you could. Then you had all summer, winter, and fall to fix, section, stain, and analyze your data. For example, Hilde Mangold's work on "the organizer" in Hans Spemann's laboratory took two breeding seasons to finish. The first group of experiments did not give definitive results, and she had to wait until the next spring's rain brought new clutches of eggs. This slower timetable may have been advantageous from the perspective of women who wanted both to do science and to raise children: one could more easily become as good an embryologist as any man and still tend to one's family. But also, as C. H. Waddington noted, other biological sciences (especially genetics) gave results much faster.[16] Since (then, as now) the number of publications counted toward tenure and promotion, men might see embryology as a difficult way to earn a living, and therefore, women might have more readily found viable careers doing this kind of biological work.[17]

Furthermore, as anybody who has worked with embryos knows, embryology, especially as it existed until the age of molecular techniques, demands fine motor skills. Manual dexterity was not just important—it was essential. One had to love precise and detailed movements with needles. One teased out pieces of somites, regions of notochords, even individual cells with one's needles and one's fingers. Because women of the time were encouraged to master needlework and other such crafts, these practical factors may initially have been significant for encouraging some women to enter a scientific field that required the same skills.

ENTRY AND SUCCESS IN DEVELOPMENTAL BIOLOGY: SALOME GLUECKSOHN WAELSCH AND C. H. WADDINGTON

Exploring the areas we have described thus far would give us even more historical information about how questions of gender related to the early involvement of women in developmental biology—specifically, embryology. But another question about women's participation remains: though many newly trained women scientists in the period from 1900 to 1940 pursued embryologically oriented fields, were they uniquely flourishing there—and why or why not? Examining in

more detail the early careers of two developmental biologists—Salome Gluecksohn Waelsch (1907–; hereafter referred to as Waelsch, although she published under various names) and C. H. Waddington (1906–75)—is instructive for understanding the professional world faced by early-twentieth-century developmental biologists, both men and women, and how gender shaped the way particular individuals negotiated places for themselves in this world.

By her own admission, Waelsch "wasn't planning to be a scientist" when she began university training. In school, though Waelsch was a very good student and had at least one woman teacher she "really respected and loved," she also had to endure the persistent anti-woman and anti-Semitic taunts of her classmates. College in Konigsberg was a welcome relief, and she originally intended to become a humanist: a Classics teacher. But like other women developmental biologists of her generation, Waelsch first came to her career in science, not because of an innate passion for the subject, but because she thought it would be the most practical route to a desired career in teaching. Once Waelsch decided to study biology, a combination of fate and persistence led her to doctoral studies. In order to earn a living to supplement her scholarships, she became a tutor to a family in Berlin. The family asked Waelsch if she would consider moving with her charge to a smaller town: "I was asked to choose a town. I chose Freiburg, because by that time I had become interested in developmental biology."[18] The University of Freiburg was the home of Hans Spemann, an already distinguished experimental embryologist and soon (1935) to be Nobel laureate for his work with "the organizer."

Not unlike other women who entered graduate programs in biology about this time, Waelsch characterizes her first experience with the world of professional academic science as "stimulating to the utmost"[19] but "negative in essence."[20] Spemann proved a reluctant teacher and an impossible mentor. Though Waelsch thought Spemann's embryology was "very exciting"[21]—as compared to genetics, which "was not my thing"[22]—she found him to be "old and an anti-Semite, and also a strong anti-feminist to participants in his experiments. He was not very eager to take me in."[23] In practice, this meant that although Spemann accepted her as a student, he assigned her "a rather boring descriptive study of limb development" which he hoped would provide the basis for some exciting experimental work on the roles of ectoderm and mesoderm in neural patterning. The important projects, Waelsch remembers, were assigned to "a young man who became an object of Spemann's love at first sight and who remained his favorite pupil."[24] Interestingly, Waelsch was not Spemann's first

or only female graduate student. To this day she frequently comments that some of the most important work to come out of the Freiburg laboratory was completed by similarly mistreated women scientists: specifically, Hilde Mangold's work on the organizer and Else Wehmeier's experiments on embryonic induction.[25]

Although she felt that Spemann discriminated against her intellectually because she was a woman, gender did not determine the division of labor in the laboratory's practical work. For example, during amphibian breeding season (three to four months in early spring), Waelsch made it her "ambition that there would not be a minute in the twenty-four hours of the day that I would not have spent in the lab at some point."[26] But she distinctly remembers these efforts were collaborative among herself and her male junior colleagues: "all of us worked day and night and we shared results, interpretations, etc."[27]

Also, and perhaps more significantly, when it came to critically evaluating the ideas and methods of their senior mentor, nearly all the students—male and female—expressed a belief that Spemann's work was too narrow. Viktor Hamburger, Spemann's senior graduate student and Privatdozent and Waelsch's de facto supervisor, formally arranged joint seminars with the Department of Philosophy to counteract this narrow thinking, and he made sure that the students obtained some introduction to the principles of genetics and how they relate to embryology.[28] Along these lines, Waelsch relied on several supportive male scientific colleagues who proved powerful intellectual and personal resources in her Freiburg years. In addition to her laboratory colleagues Hamburger and Oscar Schotte, she formed a close personal and professional friendship with British biologist C. H. Waddington, who came as a visitor to Spemann's laboratory in 1931. This was the year during which Waelsch had begun to mistrust her mentor's vitalist explanations and to have her own "thoughts about the role of genes and their possible activation in the [developmental] induction mechanism."[29] From Waddington, Waelsch says, she "received much encouragement and infinite stimulation in thinking about problems of development . . . Waddington remained one of my closest friends until the time of his death."[30]

Waddington's own entrée into developmental biology was very different from Waelsch's, although equally circuitous. He graduated from Sidney Sussex College, Cambridge, in 1926 with a degree in geology and began a Ph.D. thesis in paleontology. As Edward Yoxen has noted, this represented "a very classical and academic retreat from the scientific service of an expanding international industry."[31] But Waddington had supreme self-confidence and a decidedly philosophical

bent gleaned from years as a member of the progressive Cambridge-based Biotheoretical Gathering (whose regular participants included such distinguished scientists as Gregory Bateson, Evelyn Hutchinson, and Joseph Needham). As a result, unlike Waelsch, he was perceived as ambitious and even as something of a *Wunderkind.* He identified himself as a student of "diachronic biology"—"embryology-genetics-evolution which again form a group whose interconnections are obvious and unavoidable"[32]—though he never obtained his Ph.D. Instead he migrated from work on chick embryo culture (with Dame Honor Fell) at Strangeways Research Laboratory in England to work on amphibian neural induction at Otto Mangold's laboratory in Berlin.

But by the time Waddington came to Spemann's laboratory, his wandering had paid off. He had successfully presented his chick embryo work at the International Congress of Experimental Cytology in Amsterdam, and he was actively seeking research areas in which he could combine his traditional embryological expertise with new molecular and genetic approaches. He continued the chick project in collaboration with biochemists Joseph and Dorothy Needham, in the hopes of identifying the active agent of embryonic induction, and along these lines, he attracted the attention (and support) of the Rockefeller Foundation. But by 1938, this collaborative effort had stalled and Waddington decided to travel to the United States to visit several genetic and developmental research groups. One of the first of these was L. C. Dunn's mammalian genetics group at Columbia University, where Waddington renewed his acquaintance with Waelsch and her work.

Waelsch's developmental work had by this time taken a decidedly genetic turn—in part because of her scientific interests in this conjunction and in part because of contingent historical circumstances. The completion of Waelsch's Ph.D., in 1932, coincided with Hitler's rise to power in Germany and—as for many German scientists and intellectuals—this proved a turning point in her career. In early 1932 she started to look for German postdoctoral positions where she could pursue her interest in the border between genetics and embryology, but she met with resistance. In Richard Goldschmidt's laboratory in Berlin, for example, Waelsch was flatly turned away by Curt Stern (Goldschmidt's assistant), who told her, "You, a woman and a Jew—forget it!"[33] In 1933 she met and married Rudolf Schoenheimer, one of Germany's most promising young biochemists. Schoenheimer strongly supported Waelsch's scientific career, but in private they agreed "that it would be extremely difficult. . . . [O]ur ambitions [to become a dual career couple] were not terribly high."[34] Thus when he was offered a position at Columbia University's College of Physicians and Surgeons,

the pair immediately left Berlin for New York. In August 1933, after having spent six months "in a corner . . . without a job or a desk" in Columbia embryologist Robert Detwiler's laboratory, she met Columbia mouse geneticist L. C. Dunn at a dinner party. Waelsch recalls: "He was interested in my experiences and my training and he invited me to come to his laboratory, though he said he couldn't pay me. He had no money."[35] She saw it as a good intellectual opportunity to learn genetics.[36] It would be three years before she was officially appointed as research associate at Columbia—at an annual salary of $1,500. In the meantime, she set about retraining herself with a new complex mammalian system: the mouse.

Waelsch recalls that the atmosphere at Columbia was a far cry from the one she had experienced in Spemann's lab. To begin with, "Dunny" (as Waelsch eventually learned to call him) was politically committed and "progressive," despite the fact that he also "never met a pretty girl that he didn't pursue."[37] Dunn was a leader in the attempts to rescue German Jewish biologists and find positions for them in the United States. Also, unlike in Freiburg, the work itself was not communal: "I learned it [genetics] really by working with the animals. . . . You see, there really was no group, you know? Dobzhansky was in one corner, way back, and Dunn was in the other corner. There was very little contact."[38] Her makeshift office was located between the mouse room and Dunn's office, and each day's contact with the mammals, though "intriguing," brought new technical challenges.[39] Interestingly, Waelsch felt that neither her biology nor her socialization had especially prepared her for these challenges, but this did not limit her scientifically: "I was never particularly good with my hands, but I was perfectly able to do whatever was needed."[40] Waelsch and Dunn's subsequent collaboration consisted primarily of work on so-called T-mutation mice (a dominant mutation called *Brachyury* wherein the heterozygote T/+ mice had short tails, and the homozygous mutants died in utero), and their experiments are now hailed as the beginning of developmental genetics.[41]

Waelsch credits Dunn for the foresight to exploit the T-locus as a model system for genetic study,[42] but she was attracted to T-locus work for a different reason: because it showed "numerous and unorthodox aspects of genetic behavior" and was "unwilling to conform to the expectations of conventional genetics." For Waelsch, the real beauty of the T-locus was that it embodied a complex interaction of the structures and processes that were central to many important biological questions, including development: "[In the] T-complex[,] . . . relevant genes were shown to affect a variety of systems, thus creating a diver-

sity of problems including those of genetic transmission, recombination, gene action, pleiotropy, evolution, genetic control of development, and spermatogenesis. Such a complexity of effects presented a unique situation as well as opportunity, and raised questions of gene structure, organization, and expression, many of which have remained unanswered to this day."[43]

Moreover, the data from the observations on mutant mice fit well into the organizer project which Spemann had forbidden her to pursue in Freiburg.[44] Indeed, from her perspective, Waelsch rejects the explanation that her interests were marginal to the mainstream of experimental biology.[45] She felt that others—both male and female—shared her interests and her aesthetics: "during the middle and late 1930s . . . I witnessed the expression of a strong liaison between embryology and genetics . . . and I believe that it may be due in large part to my own close contacts with particular people."[46]

One of these people was Waddington, who visited Dunn's laboratory and was so convinced of the importance of Waelsch's mouse studies that in 1939 he, too, sought to combine genetics and development through a collaborative project on *Drosophila* wing deformities with T. H. Morgan's Caltech genetics research group. What initially motivated this work was Waddington's desire to demonstrate that the embryologist and the geneticist were studying the same phenomena: "In the late thirties I began developing the notion that the process of becoming (say) a nerve cell should be regarded as the result of a large number of genes which interact to form a unified 'concrescence.' "[47] Though many embryologists of the 1930s were wary of what Ross Harrison deemed geneticists' *Wanderlust* for developmental problems, Waddington forged ahead, and between 1938 and 1940 he wrote two textbooks and two review articles concerning the developmental action of genes. This was quite a presumptive undertaking for a thirty-three-year-old geologically trained embryologist who had yet to publish his first paper in genetics. But while perhaps full of bravado, Waddington's vision was strikingly similar to Waelsch's: he sought to identify neither the inducer nor the mechanism of gene action but "the whole complex system of actions and interactions which constitute differentiation."[48]

These overlapping biographical narratives highlight both similarities and differences in the early-twentieth-century experience of men and women practitioners of developmental biology. Clearly, though Waelsch's and Waddington's respective training was very different, their experiences led them to a common vision of developmental biology as a discipline that embraced both embryological and genetic prac-

tices. But professionally or practically, one would be hard pressed to argue that Waelsch in any way benefited from being a woman, an outsider, or a person with what we would today call interdisciplinary interests. She did not conceive of her project or her skills as "woman's work," but Columbia clearly did, and predictable institutional mechanisms consequently ensured that her work would be perceived as marginal. It was not until 1953, nineteen years after she began her T-locus work, that she finally obtained an independent appointment there and even then it was not in genetics but as a research associate in the Department of Obstetrics and Gynecology, at the College of Physicians and Surgeons. By contrast, in 1944, only six years after his first publications in genetics, Waddington's boundary crossing was rewarded: he was chosen to head up England's National Animal Breeding and Genetics Research Organization. From here, "he set out what he thought were the important strategic questions in biology." Not everyone listened, but at least he had a sanctioned forum.[49]

At the same time, this exercise suggests that gender might be used to understand the social and intellectual history of developmental biology more broadly—namely, to investigate the disciplinary boundary between developmental biology and genetics. Keller suggests that we pay attention to the cultural symbolic work of gender, and here we find Harrison's *Wanderlust* rhetoric particularly instructive. Regardless of how the practitioners themselves thought of their work, our historical understandings of their experiences would clearly benefit from a more systematic analysis of the rhetorical coding of genetics as "male" and embryology as "female" during this early period.[50]

One particularly fruitful avenue in this regard might be analysis of aesthetics. The emphasis on complexity over simplicity is a traditional characteristic of embryology that separates it from the aesthetics of genetics. The relationship between the aesthetic dimensions of embryology and feminism are briefly explored by Gilbert and Faber.[51] Whether scientists enter certain areas because of aesthetic considerations is a relatively unexplored question, but because aesthetics is infused with gender, it may contribute significantly to the recruitment and sustaining of either men or women in particular areas of science.[52] With such a rubric, we might more carefully generalize about how the kind of ambivalences and boundary crossing that characterize the careers of people like Waelsch, Waddington, and Nüsslein-Volhard map onto the kind of ambivalences embodied by biology itself during this critical period.[53] In other words, we would further illuminate the relationship between the problem of "men and women in biology" and "gender and biology."

WOMEN IN CONTEMPORARY DEVELOPMENTAL BIOLOGY: OPPORTUNITY, COMMUNITY, AND THE FEMINIST CRITIQUE

Meanwhile, however, Keller's injunction points to a more obvious place to explore how gender has shaped developmental biology and the experiences of its practitioners: specifically, the historical concordance of the postwar influx of women into developmental biology with feminist critiques of the field's mainstream scientific ideas. In the 1960s, the women's liberation movement opened up new possibilities for women in the professions. Medical schools, law schools, and even science departments began to accept women into their programs and to hire women as full-time faculty members. Universities that were slow to make the change found that talented women were being drawn away. Women who were interested in the sciences could attend the prestigious schools, and the politics had changed. Rather than become schoolteachers, these women were told (by Marlo Thomas if no one else) that they could be anything they wanted to be—even full professors and PIs.

As Waelsch's career illustrates, those who were already in developmental fields benefited from these developments. Bryn Mawr biologist Jane Oppenheimer, Waelsch's good friend and one of the few women in her cohort to have an official faculty position (albeit at a women's liberal arts college), called Waelsch's work to the attention of anatomist Ernst Scharrer. Scharrer had left his position at the University of Denver to organize his own department at the newly created Albert Einstein College of Medicine in New York: "he saw a chance there to do away with academic prejudices, e.g. against women on faculties of universities and medical schools."[54] Waelsch was among three of Scharrer's first appointments—all of whom were women. Within three years, she was promoted to full professor, and in 1963 she became the first chair of the newly separate Department of Genetics.

Beyond general trends regarding more women participating in science (and the paid professional workforce more generally), the presence of women like Waelsch in prominent places likely attracted more women to this particular type of biological work—a phenomenon Keller calls the "Jewish violinist from Odessa effect."[55] And Waelsch was hardly the only woman. By the mid-1960s, the ranks of women developmental biologists included scientists such as Ruth Bellairs, Anna Ginsburg, Anne McLaren, Kirstie Lawson, Nicole Le Douarin, Hephizibah Eyal-Giladi, and Mary Rawles. Anecdotal evi-

dence suggests that the prominence of these women in their respective areas of developmental biology was influential in attracting younger women into these areas. Indeed, by 1963, half the papers in the *Journal of Experimental Embryology and Morphology* were published by women. It was relatively easy in developmental biology to be trained by a woman (or by a man who had been trained by a woman); there may not be other areas of biology where this can be said. (For instance, SFG was trained as a doctoral student in mammalian developmental genetics by Barbara Migeon; his postdoctoral advisor in mammalian developmental biology was Robert Auerbach, a feminist and former student of Salome Gluecksohn Waelsch.)

To the extent that women scientists wanted to provide a supportive atmosphere for each other, the Society for Developmental Biology (SDB) had the resources and the resourceful women to make collective consciousness—and collective action—possible. For example, Winifred Doane, who served as one of the officers of the Women's Caucus of the SDB, writes that the SDB had "the acceptance of women members on a par with men, e.g. women were included among the officers and chairs of committees as well as given equal visibility in terms of platform presentations at the symposia. This went as far back as the early 1960s, even before the women's movement got underway. . . . I felt that other women of the SDB were very supportive at times when I really needed some moral support."[56] Biologist-turned-science-studies-scholar Donna Haraway remembers this group in the early 1970s as being proactively feminist and later becoming more career oriented. The members of the Women's Caucus included Mary Clutter, who is now assistant director of biological sciences at the National Science Foundation and who has been very active in the Association for Women in Science (AWIS). She became influential in the development and maintenance of policies aimed at attracting and retaining women in biological sciences. Still another member of this group was Susan Goldhor, who wrote a pamphlet entitled "How to Get a Job," which was distributed at the SDB meetings. It is still useful as an eye-opener for naive graduate students. Dorothy Skinner, Elizabeth Hay, Sheila Counce, Virginia Walbot, and Marie Di Berardino were also prominent biologists who were members of the caucus. These names will be familiar to developmental biologists. They constitute another formidable cross section of the field.

Besides getting more women into developmental biology, or into more prominent positions in this field, feminism was also an important resource for mounting a successful project to transform the nature of

what counted as knowledge. The first of these projects was an effort to degender the vocabulary of developmental biology. In this critique, the scientific data themselves have not been questioned so much as the types of questions thought important and the interpretations drawn from experiments and observations. Several individuals and groups have scrutinized this area and have written excellent critiques of its language, its narratives, and its interactions with society.[57] Many of the individuals who have written feminist critiques of developmental biology are themselves trained as biologists. Thus, developmental biology has seen a remarkable reform-from-within. In almost all of these instances, feminist critiques were used in an attempt to make the science "better." Feminists' critiques were used to control for social assumptions and were seen as a tool to bring interpretations back in line with the scientific data. Just as a scientist would control for temperature, pressure, and solvent effects, so the scientist should also control for social biases and cultural assumptions. The Biology and Gender Study Group has called this "controlling for social biases"; Sandra Harding calls it "strong objectivity."[58]

In the last twenty years these critiques have been particularly visible. For example, the Biology and Gender Study Group looked specifically at stories of fertilization and how the sperm and the nucleus are given masculine attributes while the egg and the cytoplasm are made to stand for women. Emily Martin looked at the language being used to describe menstruation, oogenesis, and spermatogenesis, and she came to similar conclusions about how cells became surrogates for men and women. Ruth Hubbard, the Biology and Gender Study Group, and Evelyn Keller have criticized the language being used to represent the genetic mastery over the cytoplasm.

But this program for purging sexist language from developmental biology may be traced back much earlier, to the founding years of the Women's Caucus of the SDB. In 1976, this group published a remarkable pamphlet called *Sexisms Satirized.* As its preface states, "It was made possible through the generous contributions of material from SDB members of both genders. . . . Vexed by recent statements in the biological literature which had sexist overtones, the Women's Caucus decided that satire would be the most effective approach to counteract such remarks. Hopefully the authors quoted here will be persuaded to reassess their objectivity in future publications and the awareness of scientists in general will be somewhat heightened."[59] This pamphlet is noteworthy for many reasons. First, it is one of the very earliest feminist critiques of biology, written before the better-known early analyses of Haraway, Bleier, Hubbard and colleagues, and Gilbert.[60] It is

"In all systems that we have considered, maleness means mastery; the Y-chromosome over the X, the medulla over the cortex, androgen over oestrogen. So physiologically speaking, there is no justification for believing in the equality of the sexes; *Vive la différence!*"

Figure 4.2 Cartoon from *Sexisms Satirized,* published by the Women's Caucus of the Society for Developmental Biology, 1976. The quotation being satirized is from R. V. Short in *Reproduction in Mammals,* book 2, page 70, C. R. Austin and R. V. Short, eds. Reproduced courtesy of the Society for Developmental Biology.

even antecedent to Pauline Bart's 1977 chapter in *Biology as a Social Weapon.*[61] Second, this critique of biology uses irony to make its point. It does not give a philosophical justification or an exposition on the roles of gender in science. Rather, it just quotes *verbatim* the offending text and uses a cartoon to illustrate the point. Figure 4.2 shows one example of the material in this book. This example also demonstrates the type of sexism present in some areas of developmental biology. Third, this pamphlet is a collaboration among women and men; the inclusion of men in the formulation of feminist critiques has been characteristic of developmental biology.[62] Fourth, this was an internal critique, written by scientists for scientists. The critique was couched in friendly terms and was done in the name of better science. This also became a characteristic of the feminist critiques of developmental biology.

The second feminist project in developmental biology has been to perform critiques on various research programs. Again, like the language critique, this project is being done largely within developmental

biology by developmental biologists, and it seeks to bring the rhetoric of scientific programs back in line with what the data indicate. One of the most important of these programs has been the critique by Eva Eicher and Linda Washburn of sex determination stories.[63] They pointed out that the standard story was that the default state of sex determination was femaleness, and that maleness was femaleness with something extra. This idea went right back to Aristotle's notion of sex determination that saw females as incomplete men. (And this notion was parodied in the *Sexisms Satirized* brochure.) Eicher and Washburn showed that this story was believable only if one confused primary and secondary sex determination. If you castrate a mammalian embryo, its phenotype becomes female. But that is secondary sex determination and has nothing to do with whether the bipotential gonad rudiment becomes a testis or an ovary. Primary sex determination is actually a bifurcating path, and both testis and ovary formation are active, gene-directed events. However, because of the earlier confusion, "sex determination" was almost entirely synonymous with "male determination," and the scientific research program was to identify testis-forming genes. Ovary-forming genes were not looked for until the 1990s, and two have recently been discovered. Feminist critiques of specific areas in developmental biology have also criticized certain research programs in hormones and brain development (Ruth Bleier, Anne Fausto-Sterling) and molecular biology (C. H. Waddington, Stuart Newman, Brian Goodwin, Ruth Hubbard, Evelyn Fox Keller, Bonnie Spanier). Numerous men are involved in these critiques, and each of these critiques has been advanced in the name of making the science more rigorous.

Both these programs—to change the vocabulary of the discipline and to criticize research programs that have bent science to social norms—have had large, although not complete, success, and these critiques have made their way into the teaching literature of the field. For example, one of us (SFG) writes a mainstream textbook which has been widely used in the field for the past decade. This text refers to and makes use of the above-mentioned critiques of biology as well as the analyses of science studies scholars such as Londa Schiebinger, Susan Bell, Donna Haraway, and Cor Van de Weele.[64] In the pamphlet *From Egg to Adult,* published by the Howard Hughes Medical Institute, the interactions between sperm and egg are described as a dialogue wherein the egg is seen as an active participant in the fertilization process. Similarly, the article on sex determination in this pamphlet states explicitly, "Becoming female is not a default pathway."[65]

CONCLUSIONS

The question we began with was, Have women made developmental biology a "feminist science"—or has feminism changed the means by which we do developmental biology in other ways? Our preliminary answer is a qualified "yes" to both aspects. Certainly, the main agenda of the Women's Caucus of the SDB has been met. Women scientists are no longer confronted with the expectation that the highest rank to which they can reasonably aspire is that of a senior research associate. This success, of course, is not peculiar to developmental biology. However, it certainly can be said to be due to feminism in professional terms, because particular women actively fought for policy changes in funding and representation.[66] Thanks to policies at the National Science Foundation (developed by scientists such as Mary Clutter of the Women's Caucus of the SDB), women became more prominent at meetings, and women were able to present their research more visibly. In developmental biology, there was no problem in finding women to chair sessions and give plenary sessions. Some of the most well known investigators in the field have been women. In several instances, the status of women scientists changed from one of "soft money" to one of tenure track, following the foundation's recognition of their scientific contributions.[67]

In the SDB, "once it became clear that the Society truly did support its women members, the need for the caucus evaporated. . . . Betty Hay became president of the Society and continued the tradition of supporting its women members."[68] But the equality that had been envisioned did not materialize, and the SDB formulated panels to deal with "Women's Issues." These mainstream panels have often been aimed at equalizing the practical education that men might be given by mentors in areas of negotiation, campus politics, and grant writing. In one session (held before a packed auditorium at the University of Wisconsin, Madison), graduate students and postdoctoral fellows (of both genders) were taught how to negotiate and what to expect. It was brought up that this was something that men often were told, but until recently women were just made to feel grateful for having been accepted by the university at all. One woman postdoctoral fellow urged women to be sure to negotiate for a parking space close to the laboratory, because the new recruit could expect to be there at weird hours. At another recent SDB meeting, University of Michigan professor Kathryn Tosney was given a major evening lecture session to explain the "rules behind the rules" of tenure. What is important is that these

sessions were not considered "extra," nor were they expected to be attended only by women.

In a related project, women have been protesting against the current state of tenure evaluation, wherein a woman is expected to produce her best science precisely in those years when she might be raising a young family. Princeton developmental geneticist Shirley Tilghman has been the most articulate spokesperson for that cause, and her essays in the *New York Times* sparked great debate and perhaps even some changes.[69] She relates these changes to the numbers of women entering the field, noting: "There is only one solution and that is the recruitment of more women into science. Numbers really matter. When women reach a critical mass, the cultural barriers naturally begin to slip away."[70]

We believe that feminism has indeed made a difference in developmental biology in several ways. First, large numbers of women have not only entered the field but have become its exemplars both scientifically and professionally. Feminism is challenging the politics of science more broadly and the ways in which hiring and promotion are done, and these changes have been incorporated into developmental biology in many prominent ways. Second, feminism has at least partially succeeded in changing the knowledge produced. The vocabulary of the field has been transformed, resulting in a less sexist, less culturally biased, and more scientifically congruent view of the world. Further, this shift has challenged and in some cases changed the ways the field's practitioners have viewed sex determination, fertilization, and brain development. If feminism succeeds in its internal critique of the discipline, this will be an important success, because developmental biology, like primatology, is in the business of telling us who we are and how we came to be. If it succeeds in changing the politics of science, this will also be important, for as Tilghman has written: "The reason we care so much about this subject is that science is an extraordinary field. I know of few other professions where the excitement that brought you to the field is sustained over so many years. It would be a tragedy to exclude women from all this fun."[71]

We also envision ways in which this transformed developmental biology can inform how we understand its history. Methodologically, developmental biology recognizes that what works for one organism may not work for a closely related organism: no one scheme explains all the data. Similarly, what makes one woman a scientist is not necessarily what would make another woman a scientist; what is an active agent in one set of circumstances may be poisonous in another time or place. Developmental biology also teaches us that in the determination of mammalian cell fate, context is critical. Whether a cell becomes

a skin cell or a nerve cell, cartilage or muscle, is determined by the other cells it meets. A cell is not intrinsically programmed. Who we meet—our friends, our parents, our teachers—are critical. Finally, developmental biology recognizes what it calls a "community effect": numbers matter.[72] Groups can respond to stimuli differently from isolated individuals. This is also important for historians to consider.

We believe, however, that more individual case studies (along some of the lines we suggested) will be broadly instructive for generating historical comparisons that help explain the cultural symbolic meaning and power of gender in this field. Waelsch and Nüsslein-Volhard, for example, are two very different people: the only real constant in their situations was being perceived as women. Such a perception endows one with a certain recognition of one's body, one's society—and of certain privileges and certain constraints, though these differ from place to place, time to time, household to household, laboratory to laboratory. Multiple situatedness also works in different ways at different times. Where political and social upheavals permitted the German woman Nüsslein-Volhard to be trained in particular areas of biology and to act where she felt she could make the most meaningful contributions to developmental biology, different political and social upheavals constrained the German Jewish woman Waelsch to be at the intersection of mammalian development and genetics, a place where she could work but where few other people were working. Nevertheless, as a result of the more recent feminist critiques, the contemporary culture of developmental biology brings its own perceptions of women and gender to bear on her intervention. Waelsch's article "In Praise of Complexity" thus becomes evidence that having more women in the field makes for different science. In this way, gender clearly impacts community understandings and community behavior—for both scientists and historians of science. From a historian's perspective, then, understanding this effect is most important for understanding how the stories we write about developmental biology's past simultaneously reflect and shape our understandings of the roles women and feminism should play in its future.[73]

NOTES

We wish to thank Dr. Winifred W. Doane for her letters, her insights, and her support; Dr. Ida Chow, business manager of the Society for Developmental Biology, and all those researchers who answered our questions concerning the role of the Women's Caucus of the SDB; Dr. Thomas Vogt, whose scientific vision and historical determination helped make possible a valuable new oral history of Salome Waelsch and Anne McLaren; and, finally, the editors of this volume, for

a stimulating symposium at Princeton University and the constructive scholarly exchanges that developed from it.

1. Evelyn Fox Keller, "Developmental Biology as a Feminist Cause?" *Osiris* 12 (1997): 18–19.
2. Ibid., 18.
3. Ibid., 23.
4. Ibid.
5. Ibid., 24.
6. The distinction between developmental biology and embryology is a very loose one. The term "developmental biology" can be said to have originated twice. The first time was in the 1950s, when John Berrill and Paul Weiss introduced it for the title of Weiss's journal, *Developmental Biology.* The term was meant to connote the fact that development includes more than embryology. It also includes the regeneration, the colonial animal development, and other developmental processes that occur in the adult (such as blood formation). The second time was in the mid-1960s and occurred for the opposite reasons. This time the term was meant to integrate embryology with cellular and molecular biology. The term acquired popularity through the serial *Current Topics in Developmental Biology,* which was expressly devoted to a cell and molecular approach to development. The Society for Developmental Biology was called the Society for the Study of Development and Growth until 1965. See Scott F. Gilbert, *A Conceptual History of Modern Embryology* (New York: Plenum Press, 1991), ix.
7. Diane Paul, *Controlling Human Heredity: 1865 to the Present* (Atlantic Highlands, NJ: Humanities Press, 1995), 55.
8. Margaret W. Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore, MD: Johns Hopkins University Press, 1982).
9. See Jane Maienschein, *One Hundred Years Exploring Life, 1888–1988* (Boston: Jones and Bartlett, 1989).
10. Garland Allen, *Life Science in the Twentieth Century* (New York: John Wiley and Sons, 1975), chap. 1.
11. See Maienschein, *One Hundred Years,* 157.
12. Robert E. Kohler, *Lords of the Fly: Drosophila and the Experimental Life* (Chicago: University of Chicago Press, 1994), 96.
13. See Karen A. Rader, "The Mouse People: Murine Genetics Work at the Bussey Institution, 1909–1936," *Journal of the History of Biology* 31 (1998): 327–54.
14. Amy Sue Bix, "Experiences and Voices of Eugenics Field-Workers: 'Women's Work' in Biology," *Social Studies of Science* 27 (1997): 625–68.
15. See Scott F. Gilbert and Marion Faber, "Looking at Embryos: The Visual and Conceptual Aesthetics of Emerging Form," in *The Elusive Synthesis: Aesthetics and Science,* ed. Alfred I. Tauber (Dordecht: Kluwer Academic Publishers, 1996), 125–51; and Scott F. Gilbert, "Bearing Crosses: The Historiography of Genetics and Embryology," *American Journal of Medical Genetics* 76 (1998): 168–82.
16. C. H. Waddington, *New Patterns in Genetics and Development* (New York: Columbia University Press, 1962).
17. Rossiter, *Women Scientists in America.*
18. Transcript of Oral History Interview with Salome Waelsch and Anne McLaren, by Karen Rader and Thomas Vogt, Princeton, NJ, June 1997 (hereafter OHI/97), 8.
19. Salome Gluecksohn Waelsch, "The Causal Analysis of Development in the Past Half Century: A Personal History," *Journal of Development* supplement (1992): 1.
20. Salome Gluecksohn Waelsch, "The Development of Creativity," *Creativity Research Journal* 7 (1994): 249.

21. OHI/97, 41.
22. Ibid., 54.
23. Ibid., 4
24. Waelsch, "Causal Analysis of Development," 1.
25. See Waelsch "Causal Analysis of Development" and OHI/97.
26. OHI/97, 50.
27. Waelsch, "Causal Analysis of Development," 1.
28. Ibid., 2; OHI/97, 45; Salome Gluecksohn Waelsch, "Viktor Hamburger and Dynamic Concepts of Developmental Genetics," in *Studies in Developmental Neurobiology: Essays in Honor of Viktor Hamburger,* ed. W. Maxwell Cowan (New York: Oxford University Press, 1981), 44–52.
29. Waelsch, "Development of Creativity," 250.
30. Waelsch, "Causal Analysis of Development," 2.
31. Edward Yoxen, "Form and Strategy in Biology: Reflections on the Career of C. H. Waddington," in *A History of Embryology,* ed. T. J. Horder, J. Witkowski, and C. C. Wylie (Cambridge: Cambridge University Press, 1986), 309–29, quotation on 311.
32. C. H. Waddington, *Organisers and Genes* (Cambridge: Cambridge University Press, 1940), 1. For more on this issue, see Scott F. Gilbert, "Induction and the Origins of Developmental Genetics," in Gilbert, *A Conceptual History of Modern Embryology,* 181–206; and Scott F. Gilbert, "Epigenetic Landscaping: Waddington's Use of Cell Fate Bifurcation Diagrams," *Biology and Philosophy* 6 (1991): 135–54.
33. Waelsch, "Development of Creativity," 250.
34. OHI/97, 60.
35. Ibid., 63.
36. Ibid., 70, 73.
37. Ibid., 199 (see also McLaren's comments on the same page).
38. Ibid., 72.
39. Ibid., 76.
40. Ibid.
41. See Gilbert, "Induction and the Origins of Developmental Genetics."
42. Waelsch, "Causal Analysis of Development"; see also S. Waelsch, "In Praise of Complexity," *Genetics* 122 (Aug. 1989): 721–25.
43. Waelsch, "In Praise of Complexity," 721.
44. See also Gilbert, "Induction and the Origins of Developmental Genetics."
45. OHI/97, 166.
46. Interview with Salome Waelsch in *The Outer Circle: Women in the Scientific Community,* ed. Harriet Zuckerman, Jonathan R. Cole, and John T. Bruer (New York: W. W. Norton, 1991), 71–93.
47. C. H. Waddington, "The Practical Consequences of Metaphysical Beliefs on a Biologist's Work: An Autobiographical Note," in *The Evolution of an Evolutionist* (Ithaca, NY: Cornell University Press, 1975), 3.
48. Waddington, *Organisers and Genes,* 3–4; see also Gilbert, "Induction and the Origins of Developmental Genetics"; and Gilbert, "Epigenetic Landscaping."
49. Yoxen, "Form and Strategy," 323–24. Early on, Waddington himself seems to have been particularly sensitive to the marginalized position of women in biology. In a letter asking Theodosius Dobzhansky to temper some of his criticisms of a particular woman geneticist, Waddington wrote that neither Waelsch nor Barbara McClintock had an official position in line with their scientific abilities or efforts: "I think that women biologists in America have in any case a very difficult job to get themselves accepted—look at Barbara McClintock, as the most extreme case of an absolutely first-rate person who has been forced into the position of an eccentric recluse; and the positions of Salome Waelsch, Jane Oppenheimer, Dorothea Rudnick is only slightly better" (letters from Waddington to

Dobzhansky, Mar. 11 and 20, 1964 [with Dobzhansky's letter to Waddington appended, Mar. 15, 1964], Dobzhansky Papers, American Philosophical Society).

50. See Biology and Gender Study Group, "The Importance of Feminist Critique for Contemporary Cell Biology," *Hypatia* 3 (1988): 61–76.

51. Gilbert and Faber, "Looking at Embryos."

52. James W. McAllister, *Beauty and Revolution in Science* (Ithaca, NY: Cornell University Press, 1996).

53. Ronald Rainger, Keith Benson, and Jane Maienschein, eds., introduction to *The American Development of Biology* (Philadelphia: University of Pennsylvania Press, 1988), 3–11.

54. Zuckerman, Cole, and Bruer, *The Outer Circle,* 82.

55. Keller, "Developmental Biology as a Feminist Cause?" 23.

56. Winifred W. Doane, letter to SFG, Jan. 25, 1999.

57. Donna J. Haraway, *Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth-Century Developmental Biology* (New Haven, CT: Yale University Press, 1976); Ruth Hubbard, Mary Sue Henifin, and Barbara Fried, eds., *Women Look at Biology Looking at Women* (Cambridge, MA: Schenkman, 1979); Gerald Schatten and Heidi Schatten, "The Energetic Egg," *Sciences* 23 (1983): 28–34; Ruth Bleier, *Science and Gender: A Critique of Biology and Its Theories on Women* (New York: Pergamon Press, 1985); Anne Fausto-Sterling, *Myths of Gender: Biological Theories about Women and Men* (New York: Basic Books, 1985); Eva M. Eicher and Linda Washburn, "Genetic Control of Primary Sex Determination in Mice," *Annual Review of Genetics* 20 (1986): 327–60; Biology and Gender Study Group, "The Importance of Feminist Critique"; Emily Martin, "The Egg and the Sperm: How Science Has Constructed a Romance Based on Stereotypical Male-Female Roles," *Signs: Journal of Women in Culture and Society* 16 (1991): 485–501; Evelyn Fox Keller, *Refiguring Life: Metaphors of Twentieth Century Biology* (New York: Columbia University Press, 1995).

58. Biology and Gender Study Group, "The Importance of Feminist Critique"; Sandra Harding, *Whose Science? Whose Knowledge? Thinking from Women's Lives* (Ithaca, NY: Cornell University Press, 1991).

59. Winifred W. Doane, ed. (cartoons by B. K. Abbott), *Sexisms Satirized* (Pocketbook Profiles/Society for Developmental Biology, 1976).

60. Donna Haraway, "Animal Sociology and a Natural Economy of the Body Politic I and II," *Signs: Journal of Women in Culture and Society* 4 (1978): 21–60; Ruth Bleier, "Social and Political Bias in Science: An Examination of Animal Studies and Their Generalization to Human Behavior and Evolution," in *Genes and Gender II: Pitfalls in Research on Sex and Gender,* ed. Ruth Hubbard and Marian Low (New York: Gordian Press, 1978), 49–69; Hubbard, Henifin, and Fried, *Women Look at Biology;* Scott F. Gilbert, "The Metaphorical Structuring of Social Perceptions," *Soundings* 62 (1979): 166–86.

61. Pauline B. Bart, "Biological Determination and Sexism: Is It All in the Ovaries?" in *Biology as a Social Weapon,* ed. Ann Arbor Science for the People Editorial Collective (Minneapolis: Burgess, 1977), 69–83.

62. SFG, for example, received his M.A. in the history of science under the aegis of Donna Haraway, and his Ph.D. in biology in the laboratory of Barbara Migeon, two very different types of feminists.

63. Eicher and Washburn, "Genetic Control of Primary Sex Determination in Mice."

64. S. F. Gilbert, *Developmental Biology,* 6th ed. (Sunderland, MA: Sinauer Associates, 2000).

65. Maya Pines, ed., *From Egg to Adult: What Worms, Flies, and Other Creatures Can Teach Us about the Switches That Control Human Development* (Bethesda, MD: Howard Hughes Medical Institute, 1992).

66. Political mentors were also important in this process. Heinrich Waelsch,

Salome's second husband, not only was "extremely cooperative and helpful" regarding her desire to have children while continuing to pursue her T-locus work (OHI/97, 87) but also was her political mentor. By her own recollection, it was "Heini" who crystallized her resolve to approach Dunn about professional advancement at Columbia, given the vast amount of work she had accomplished (Zuckerman, Cole, and Bruer, *The Outer Circle,* 82–83). He also helped her use her newfound fame to get Columbia president Dwight D. Eisenhower to obtain a larger university apartment for them (OHI/97, 108–9).

67. M. E. Clutter, letter to SFG, Mar. 8, 1999.

68. Winifred W. Doane, letter to SFG, 1999.

69. Shirley M. Tilghman, "Science vs. the Female Scientist," Op-ed, *New York Times,* Jan. 25, 1993, sec. A, p. 17, col. 1; Shirley M. Tilghman, "Science vs. Women—A Radical Solution," Op-ed, *New York Times,* Jan. 26, 1993, sec. A, p. 23, col. 2.

70. Tilghman, "Science vs. Women."

71. Ibid. Tilghman, an outspoken feminist, was elected president of Princeton University in 2001.

72. J. B. Gurdon, P. Lemaire, and K. Kato, "Community Effects and Related Phenomena in Development," *Cell* 75 (1993): 831–34.

73. See also Margaret W. Rossiter, "The Matilda Effect in Science," *Social Studies of Science* 23 (1993): 425–41.